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Comment on egusphere-2022-814

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Referee comment on "Drivers of particle sinking velocities in the Peruvian upwelling system" by Moritz Baumann et al., EGUsphere,
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Review of "Drivers of Sinking Velocities in the Peruvian Upwelling System" by Baumann et al.

Summary

This manuscript explores the relative influences of biogeochemical and physical properties of marine particles on their sinking velocity (SV) in the Peruvian Upwelling System . An experimental setting consisting of eight mesocosms deployed off the coast of Peru allowed the production and sampling of sinking particles needed for the analyses. The effect of an upwelling event ("OMZ treatment") on particle SVs was tested via "deep water additions" consisting of the addition of nitrate-depleted water (2 levels) inside the mesocosms. This mesocosm setup has been validated in previous works and the protocols appear to have been followed thoroughly and rigorously. The sampling of the particles was done via a long 10mm tube connected to the bottom of the mesocosms and using a manual vacuum pump to suck the material all the way up to the surface lab inside a 5L bottle. Measurements of particle sinking velocities were conducted following a method developped by Bach et al. (2012) and using the FlowCAM as a settling column and video recording apparatus. Over the particle size range 40-1000µm, the SV recorded varied between 19.4±0.7 and 34.2±1.5 m d^{-1}. According to their results the authors suggest size as the main driver of particle sinking velocities, while porosity and shape (aspect ratio) exert measurable but weaker controls. The authors note the unexpected absence of relationship between the opal content of the particle and their SV.

General comments and recommendations

This manuscript is well structured, and easy to follow. The figures are clear and most of them follow a very similar model to those used in several previous publications (Bach et al., 2019; Bach et al., 2020). The statistical analyses appear very robust and are used to support most of the conclusions drawn. I acknowledge the value of the data presented here and strongly support works conducted in regions where there is a lack of observation.

However, while reading this manuscript was very satisfaying, the final impression I had was a little disappointment that some interesting aspects were not explored more deeply. The take-home messages advertised here — i.e. the absence of relationship between opal content and SV or that size is the main controlling factor over porosity and shape — remain very poor in comparison to the abundant literature that precedes this study. The main interest thus lies on the regional / local aspect of the SV data, and their possible interaction with upwelling events and the OMZ persistence. The range of SV reported here is very narrow and the conclusion could be that there is no significant SV variation despite a change in plankton community. This could be a very interesting result. I was disappointed to find in the Discussion a significant part allocated to a list comparing the SV obtained here with others. Unfortunately, either a match or mismatch of the SV observed here and elsewhere would not lead to any useful conclusion. I find such listing unecessary and pointless if all other particle properties are not considered and proved to be unvariant across studies (which is of course never the case). This brings me to my main concern. I may have missed it but I don't think there is any report on the nature of the particles sampled. Was it dominated by micro-aggregates? Fecal pellets? Large individual diatom or dinoflagellate cells? Did the category of the dominant particle pool change over time? The absence of data on the nature of the particle is an enormous obstacle to the understanding of the processes ongoing during this experiment. I assume that at least a part if not all of the images taken by the FlowCAM have been stored. Why not using these images to try and understand for example why the diatom opal content was not correlated to the SV? Images showing for example when and where fecal pellets were produced would inform on the role that zooplankton might have played here. Bretagnon et al. (2018) mentioned the abundance of fecal pellets in this region (their study is less than 50 km away). This is an important aspect because some assumptions made here on the fractal structure of the particles (see detailed comments) cannot apply to the biologically-aggregated particle such as fecal pellets. Finally, I find the extrapolation of the present results out of this particular experiment setting being a bit too confident. Results from one isolated study should be taken with caution and used to draw conclusions inside this context. Unfortunately, the relative influences of particle size, porosity, composition, structure, etc. on their SV will remain an ongoing debate for probably many years, but every little contribution is welcome, even if it does not give any useful relationship to global numerical modelers. I have no doubt that this overall good article will find its place among this large litterature and will bring a small but still important contribution.

The attached pdf includes all my detailed comments, which I hope will help the authors improve further their manuscript.

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Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-814/egusphere-2022-814-RC1-supplement.pdf